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THE JUSTIFICATION OF THE HABIT OF INDUCTION[1]

THIS paper is essentially concerned with second-level considerations in regard to the "goodness" of the habit of induction. It aims not so much to prove or disprove the reasonableness of the habit as to make out just what has and has not been accomplished in Professor Reichenbach's "justification of induction," or alleged demonstration that the habit is a good one. Professor Reichenbach's claim[2] to have solved the problem of induction which Hume could not solve has been met by the charge that this latest "justification" leaves the problem exactly where Hume left it.[3] It will be my attempt to estimate critically the force of the claim and counter-claim. As a preliminary step, I wish to make the following points.

There are two types of significant questions which may be asked concerning inductive procedure. 1. What is its character, aim, and fundamental assumption? 2. Is there any way in which it may be exhibited as a justifiable, or valid, or reasonable procedure, and if so, in what sense? The questions are obviously not independent in that the first must be answered, at least to some extent, before the second is undertaken. If the second be answered convincingly in the negative, the so-called "problem" of the justification of induction does not become a pseudo-problem, but rather a question which is answered and answered by a "no." Further, for any genuinely scientific philosophy, these questions need to be asked anew at every major change in the content and methods of the sciences. Professor Reichenbach's account of what he terms "the justification of induction" is based upon his comprehensive and thorough studies of the concept of probability in its mathe-

[1] I wish to express my debt to Professor Reichenbach in the writing of this paper. He has helped me not only in my attempt to give a fair representation of his views but also in my attempt to make the critical comment as clear as possible. However, I do not wish to suggest that he would agree throughout either with my exposition or with my comments.

[2] *Experience and Prediction*, pp. 348 ff. "Causalité et Induction," *Bulletin de la Société francaise de Philosophie*, 37e Année (1937), pp. 139 ff. *Wahrscheinlichkeitslehre*, pp. 410 ff.

[3] Everett J. Nelson, in "Comments and Criticisms," this JOURNAL, Vol. XXXV (1938), p. 356.

matical aspect as well as upon first-hand knowledge of contemporary science. Hume was not a mathematician and Hume lived in the eighteenth century, when the basic assumption of natural science seemed to be that of the existence of strictly causal laws, or invariable connections between events of certain characters. Accordingly, Professor Reichenbach does not, and could not, leave the "problem" of induction exactly where Hume left it. However, it will be well to note and to bear in mind throughout our discussion, that Professor Reichenbach has explicitly and emphatically, in every presentation of his position, expressed agreement with Hume in holding that the basic assumption of induction, can *not* be proved (or disproved) either by *a priori* or by *a posteriori* reasoning—in the latter case, at least not without circularity. "These two pillars of Hume's criticism of the principle of induction have stood unshaken for two centuries, and I think that they will stand as long as there is a scientific philosophy." [4] If, then, one interprets the problem of induction as that of proving, without circularity, the assumption of induction, Professor Reichenbach's view is that *this* problem is insoluble, or rather that the question involved is answered with a flat "no." Nevertheless, Professor Reichenbach believes that to rest with this conclusion signifies "la débacle de la théorie de la connaissance," [5] since it follows from Hume's position that "science might as well not be continued—there is no use for a system of predictions if it is nothing but a ridiculous self-delusion." [6] Further, Hume's "psychological" interpretation of induction as a habit, is, according to Professor Reichenbach, a weak subterfuge which in no way palliates the tragic consequences of the Humian position.[7] If we can not demonstrate that inductive procedure leads us to reliable conclusions we must admit that we are determined, in daily life as well as in science "par une confiance primitive en un principe qui, devant la lumière de l'intellect ne mérite aucune préférence. Admettre cette antithèse entre instinct et raison, signifierait la faillite de toute philosophie scientifique. . . ." [8] If, then, scientific philosophy is not to fail, the habit of induction must be shown to be *good* habit, that is, one "useful for the purposes of actions directed to future events." [9]

The way whereby, according to Professor Reichenbach, our trust in inductive procedure can be justified "devant la lumière de l'intellect" is marked by a reformulation of the problem in which the following three points play an important part.

[4] *Experience and Prediction*, p. 342.
[5] "Causalité et Induction," p. 131.
[6] *Experience and Prediction*, p. 346.
[7] "Causalité et Induction," p. 131. Cf. *Experience and Prediction*, p. 346.
[8] "Causalité et Induction," p. 132.
[9] *Experience and Prediction*, p. 347.

1. The approach to and solution of the problem is to be *epistemological* rather than *ontological.*[10] In other words, the solution of the problem is to be accomplished quite independently of any consideration of the truth or finite degree of probability of the assumption about the structure of Nature which is the basic assumption of induction.

2. The inductive postulate is given a more general formulation than the traditional one. Causal laws, in this generalized formulation, appear as special cases of the type of law or order in Nature which, if it existed, would guarantee success.[11]

3. The demonstration which is required to establish the goodness, or reasonableness, of the habit of inductive inference, is given an interpretation less rigid than the Humian one in the following two respects. (*a*) We need not, according to Professor Reichenbach, prove that inductive inference yields true conclusions. To be sure, if we could do this, ". . . inductive procedure would be justified, but the converse does not hold. . . ." [12] We need to prove only that inductive procedure is an approximative procedure which, through continued application, will lead to increasingly reliable results. (*b*) We need not prove that induction is a sufficient condition of success (this would involve a proof of the assumption of induction), but only that it constitutes a *necessary* condition, in the usual sense that, if we do not apply inductive procedure, then we shall not be successful.[13]

Before I state the demonstration itself, there is a major ambiguity which must be mentioned. It is perhaps responsible in part for misunderstandings on both sides of the controversy over what Professor Reichenbach's "justification" has effected. The ambiguity is this. Professor Reichenbach, in all versions of his demonstration, calls it, indifferently, the justification of "the principle of induction," of "the inductive inference," of "inductive procedure," of "induction."

While the latter three terms seem more or less synonymous, the "principle of induction" is commonly taken to mean, not the rule of procedure or method of induction, but its ontological postulate or assumption. According to Professor Reichenbach, the rule of procedure in induction is that of a continual assumption that the relative frequency of a series observed to date holds approximately for any future prolongation of the series. As the series continues

[10] "Causalité et Induction," pp. 140–141. See also, "Comments and Criticisms," this JOURNAL, Vol. XXXV (1938), p. 128.

[11] "Causalité et Induction," p. 140. *Experience and Prediction,* p. 350.

[12] *Experience and Prediction,* p. 348.

[13] Professor Reichenbach does not clearly distinguish assertions (*a*) and (*b*), though they are obviously different.

we may obtain new values for the limit, h^n, but we always choose the last h^n. The procedure is one of anticipation of the limit and later correction.[14] The principle of induction, interpreted as the ontological assumption presupposed by the procedure is clearly stated on page 40 of *Experience and Prediction* to be the postulate that there are limits (practically attainable) of the frequencies of *some* series of events, other series being reducible to these. Now, difficult as it may be for Professor Reichenbach to give the inductive postulate propositional meaning, on his own theory of meaning,[15] the meaningfulness (though not known truth-value) of the postulate is essential, it will be seen, to the demonstration that the procedure of induction is a good or justifiable one. Accordingly, Professor Reichenbach's approach is not so extremely or exclusively epistemological as to reduce an ontological assumption to a rule of procedure, but seems to be of the following moderate sort. What is to be justified is essentially the procedure or method of induction. The ontological assumption, or postulate, presupposed[16] by the procedure is justifiable only in the sense that it is the assumption presupposed by the justifiable procedure. The postulate is unjustifiable in the sense that we can not prove it true or finitely probable. (In order to avoid the ambiguity just indicated, I shall use, in discussing Professor Reichenbach's demonstration, "inductive rule," "procedure," "method" to denote the methodological meaning of "the principle of induction" and "postulate," "assumption," to denote its ontological meaning.)

I proceed to state the steps in the demonstration. It should be noted that the demonstration is a strict or tautological one, i.e., Professor Reichenbach's "justification" of induction consists in exhibiting an entailment of meanings. The first step is a definition of induction in terms of its postulate, aim, and rule of procedure.[17]

1. (*a*) The postulate of induction is the assumption that there exist, for some series of events in Nature, practically ascertainable limits of the frequency (other series being reducible to these).

(*b*) The aim of induction is to find series of events whose frequency of occurrence converges towards a limit.

(*c*) The method of induction is that of continued anticipation of the limit and later correction.

[14] *Experience and Prediction*, pp. 340, 353.

[15] ". . . *A proposition has meaning if it is possible to determine a weight, i.e., a degree of probability for the proposition.*" "Possibility" here is said to mean "physical possibility," *op. cit.*, p. 54.

[16] A procedure, rule, or method does not, strictly speaking, presuppose (in the sense of "imply") anything, since only propositions have implications. What is meant above is that the success of the procedure depends upon the truth of the assumption.

[17] *Experience and Prediction*, pp. 348–363.

2. The next step consists in showing that it follows from the mathematical definition of a limit, that the procedure of induction is a necessary condition of finding limits. It is a necessary condition in that it is an appropriate means for achieving the end or aim stated above.[18]

The demonstration is indeed an analytical one. I think that Professor Reichenbach has *proved*, in the strict sense, that if there are statistical laws, then the way of looking for them which is scientific method is the best way to find them. I do not question the demonstration, but I do question the claims which Professor Reichenbach seems to make for it and the interpretation which he puts upon it. However, before I attempt to make out what the demonstration does *not* effect, I wish to state what I think it does effect. First, the demonstration exhibits the implications of a *very good* definition of induction, a definition which is the result of a careful, realistic, scientific study of science. Secondly, in showing that, granted its assumptions, science is a logically good method of guessing, the demonstration is a corrective for that tendency of empiricists to lean over backwards in combatting dogmatic rationalism. The fact that we can not guarantee the success of science does not, of course, mean that from the viewpoint of its basic assumptions, science is a wild and arbitrary kind of guesswork or leap in the dark.

In considering the claims which Professor Reichenbach makes for his demonstration, the following points should be kept in mind. The controversy which his demonstration has provoked concerns not at all the validity of the demonstration, but rather the correctness of the interpretation of the demonstration as a "justification." Accordingly, the only way to settle the controversy is to analyze the concept or concepts of justification. And the issue, I think, is not merely verbal. To be sure, anyone can call any kind of demonstration a "justification," anyone can use the word in an entirely new way. Further the "justification of induction," if possible, would have to be relatively unique, since justifications of other procedures or habits presuppose the use of induction. However, if the "justification of induction" is absolutely unique in the sense that it presents no analogies whatsoever to the ordinary and fairly well-established meanings of "justification," it would obviously be wiser to use another term. While Professor Reichenbach recognizes and insists that the problem of the "goodness" of the habit of induction has no perfect analogy, short of itself, still his use of the term "justification" and of cases of ordinary sorts of justification to illustrate his meaning, seems to involve the assertion of *some* analogy, however slight. I propose, then, to distinguish

[18] *Ibid.*, pp. 354–356.

various sorts and degrees of justification in order to determine in what sense and to what extent Professor Reichenbach has justified induction before the court of the scientific intellect, to change his metaphor. Let us first consider the claims that have puzzled critics of Professor Reichenbach's "justification." The remarks following the demonstration, rather than the demonstration itself, have led to the charge that he has resurrected Pascal's wager or William James's *Will to Believe*.[19] These remarks seem to be of three types.

1. Even if a man does not believe in success (and the existence or nonexistence of the belief is a psychological or biological matter), he *ought* (on logical grounds) to employ the method of induction.[20]

2. If we *want* to make (reliable?) predictions, then we should apply inductive procedure, i.e., act as if we believed in success.[21]

3. We ought to "bet on" induction, because (*a*) we can neither prove nor disprove the assumption which, if proved, would guarantee success, and (*b*) inductive procedure is our best bet or wager.[22]

To consider first remarks of type 1—Professor Reichenbach's imperative here is not, of course, a moral imperative, but presumably has the import, "This is the reasonable, or rational, thing to do under the circumstances." Now, an imperative, or "ought," in connection with a procedure which is a means to some specified end is always a conditional imperative, never an absolutely categorical imperative. One of the requisite conditions is the proof (in a strict or loose sense of the term) that the procedure in question is the best means to the specified end. Professor Reichenbach has, I think, proved (in the strict sense) the existence of this condition for inductive procedure. Dr. Nelson has objected that unless we can establish the finite probability of an end, q, to speak of performing p as a means to q, let alone as the *best means*, is of questionable significance.[23] I agree with Professor Reichenbach's reply, "To show that one means is better than another, I need not show that both of them involve a finite probability of success; it suffices to show: *if there is* a finite probability of success, the first means involves a greater probability of success than the second." [24]

To illustrate the point, I prefer to alter Professor Reichenbach's case of the hungry fishermen in such a way that the finite probability

[19] This JOURNAL, "Comments and Criticisms," *loc. cit.*, pp. 355–360.

[20] *Experience and Prediction*, pp. 362, 402.

[21] "Causalité et Induction," p. 159. For what Hume said to the man who wanted to predict, see *The Treatise*, Bk. I, Part III, Section XV, especially p. 175 in the Selby-Bigge edition.

[22] *Experience and Prediction*, pp. 362–363.

[23] "Professor Reichenbach on Induction," this JOURNAL, Vol. XXXIII (1936), and especially "Comments and Criticisms," this JOURNAL, Vol. XXXV (1938), p. 357.

[24] "Comments and Criticisms," this JOURNAL, Vol. XXXV (1938), p. 130.

of success (catching fish) is not so obvious. Let us, then, substitute for the probable fish, merely logically possible mermaids. It seems at least logically meaningful to say to our hypothetical fishermen, "To cast your nets in the sea is a better means of catching a mermaid, than to sit in your boat doing nothing," or "If there is a finite probability of catching a mermaid, then casting your nets involves a greater probability of success than not trying at all."

But our illustration serves also to show that the condition which Professor Reichenbach has proved—let us call it condition (*a*)—does not suffice to warrant the assertion of the implicate of a conditional imperative. The additional requisite conditions which would afford a relatively strong type of justification for some instrumental procedure would be—(*b*) that it be known, or believed on good evidence, that the end is practically attainable in the sense that success has a finite probability (i.e., a probability separated from *O* by a finite amount, however small); (*c*) that the end be desired or wanted, the desire or want being dependent upon the fulfilling of condition (*b*) as well as upon other factors.

The fulfillment of these conditions would yield, I think, the type of justification which the scientific intellect itself demands—behavior based, not indeed upon certain knowledge of the outcome, but on *some* evidence which makes success at least probable.

A far less strict and scientific justification, warranting a far less strict and scientific "ought," would result if we omitted from (*b*) and (*c*) the factor of the known probability of success and required only:

(*b'*) that it be assumed or believed that success is practically possible, though there be no evidence to support the assumption or belief. (We should add here that evidence against the practical possibility of success must be lacking also.)

(*c'*) that the end be desired regardless of the lack of evidence for success.

A procedure justified by the existence of conditions (*a*), (*b'*), (*c'*), might be called reasonable in the following attenuated sense. Such a procedure would be consistent with the belief or assumption, though the belief or assumption itself would be quite unreasonable. However, I find great difficulty in Professor Reichenbach's notion that whether or not a man believe in success, he ought to employ the best means to the end in question. To illustrate the points which I wish to make in this connection, let us consider the following situation. (The analogy is not, of course, perfect, but I have tried to construct the case in such a way that the resemblance is as close as possible and it also has the advantage of resembling historical situations.)

Representatives of various churches can, it seems to me, prove to a man in a way at least approaching a rigorous logical demonstration that if salvation and immortal life in the company of a supreme personal spirit are practically attainable, then the performance of certain rites and the expression of certain beliefs constitute the best means of attaining the end in question, i.e., that if success here is practically possible, the behavior indicated is a necessary though not sufficient condition of success. Further, a great many people, perhaps all people, would admit that the end in question is desirable, or that they would want it if they believed it practically attainable. Suppose, now, that a man who neither actively believes nor disbelieves in success, because he can find no empirical evidence for or against, is approached by a representative of the church who proves to him the existence of condition (*a*) and then issues a categorical "ought," adding, "Even if you remain neutral or actively disbelieve in success you ought at least to fulfill the necessary conditions of success." Would the imperative have *any* relevance for our nonbeliever?

Our case will serve to illustrate another important point which concerns the nature of belief. In the middle ages, at any rate, the Church felt warranted in issuing a categorical imperative to *all* men, because it interpreted human behavior as in some sense presupposing the fulfillment of either conditions (*b*) and (*c*) or (*b'*) and (*c'*). The nonbelievers just didn't recognize the appropriate means to supreme and lasting happiness. The point which I wish to make here is that the only grounds which would warrant Professor Reichenbach's universal imperative—as a consequence of the weak sort of justification indicated above—would be Hume's account of the habit of induction and the belief to which it gives rise.

I am not sure what Professor Reichenbach means by "belief," though, in general, he suggests that belief is a personal, psychological matter which should not be mentioned in a logical justification of induction. Further, he seems to suppose that a great many people do not (or at least might not) believe in the practical possibility of the success of induction, even though they act (or *ought* to act) *as if* they did. However, in one sentence in *Experience and Prediction* Professor Reichenbach suggests a view about the belief in success (i.e., belief in the postulate of induction) which would go further than Hume went in identifying it with a habitual procedure. "There is such a belief; everyone has it when he makes inductions. . . ." [25] To a narrowly behavioristic view of belief, the fact that we do have the habit of making inductions would verify the assertion that we believe in the probability of success. Such a view

[25] *Experience and Prediction*, p. 402.

seems acceptable to the following extent. To suppose that a sane man might sincerely disbelieve in success, or just suspend judgment, and yet throughout his life continually employ the appropriate means to the end in question, would be sheer nonsense. With certain qualifications, such as the admission of the behavior that is talking as evidence for the existence of certain *felt* drives, the workable test of belief *is* behavioristic.

To summarize this part of the discussion, I have attempted to point out that (1) a relatively weak and attenuated type of conditional "justification" may be given of inductive procedure. But (2) since one of the requisite conditions is the presence of an unjustifiable belief, Professor Reichenbach's assertion of an imperative is dependent for *its* justification upon the recognition of something very much like Hume's habits. If Professor Reichenbach maintains that a psychological factor such as that of belief is irrelevant to his "justification," which is concerned with the logical relations between the logical structure of inductive inference, its assumption, and aim, and that the exhibition of these relations entails the assertion of an imperative, I should answer not by changing the point, but the words,[26] substituting "assumption" for "belief." Inductive procedure has an attenuated kind of rationality viewed in connection with its basic assumption. (This *must* be so because we infer the assumption from the procedure as what would give it rationality.) To a man who does not employ inductive method and hence does not make the assumption, an imperative would be meaningless or at least unwarranted, for it would amount to telling him "You ought to proceed in this way which involves this unprovable assumption, because this way of proceeding is the best way of attaining the end which in the assumption is assumed to be practically possible."

The second type of remark which I wish to consider is represented by the implicative proposition, *If you want to predict, then you ought to employ the inductive procedure.*

Now, an essential feature of inductive procedure *is* prediction. One of the many merits of Professor Reichenbach's account of scientific method seems to me his recognition of this fact. Induction does involve many tautologous assertions about resemblances or about probability theorems, to be sure, but so far as it is inductive inference, it involves prediction on the basis of the past, and hence

[26] I substitute "assumption" as having a less "psychological" connotation. While the issue here is doubtless to some extent merely terminological, I confess that it seems to me a very "bad" way of speaking to say that a man ought (logically) to act always *as if* he had a certain belief, though he ought not (logically) to have the belief. It seems to me a better way of speaking to say that granted a certain groundless belief and other relevant conditions, then the believer ought (in an attenuated sense of "ought") to act in a way appropriate to the belief.

is non-tautologous. However, I do not think that Professor Reichenbach in remarks of the above type intends to state merely, "If you want to predict, then you ought to predict." Accordingly, we must understand "predict" in the *implicans* to mean, "predict successfully more often than not" or "make predictions which are confirmed to a high degree more often than not." And in the implicate, of course, inductive inference designates a certain method or way of predicting.

It is remarks of the type in question, I think, which have led Dr. Nelson to ask whether Professor Reichenbach has resurrected William James's "Will to Believe." Conjoined with the asserted imperatives [27] and with remarks about the need, in practical life as well as in science, for reliable predictions, the implicative proposition suggests that Professor Reichenbach interprets the existence of conditions (a) and (c'), plus that part of (b') which consists in the lack of evidence against success, as sufficient for asserting an attenuated kind of "ought." Condition (a), namely, that the procedure in question is the best means to the specified end, has been demonstrated by Professor Reichenbach. The part of (b') in question—let us denote it by (b'')—was demonstrated by Hume. Professor Reichenbach, it should be noted, admits that, in order to assert the imperative (and hence in this sense to justify inductive procedure) the tautological demonstration—which establishes condition (a)—must be supplemented by a material proposition establishing condition (c').[28]

The points which I wish to make here have been, to a certain extent, covered in the preceding discussion. We have seen that the requirement of the scientific intellect for a justifiable instrumental procedure would indeed include a condition concerned with the existence of a want or desire, but the want or desire would be in part determined by a well-grounded belief in the practical possibility of the end. Further, the proposition stating condition c' is of the type that can not be demonstrated because it is a synthetic proposition about a psychological or biological fact. Since we read the wants and desires of men largely from their behavior, the existence of condition c' would be an aspect of the existence of the Humian habits.[29] Accordingly, again it appears that the assertion of the imperative would depend upon an acknowledgment of the more or less compulsory character of the habits.

However, remarks of the second type do raise issues which have

[27] See remarks of types 1 and 3. *Experience and Prediction*, pp. 363, 402.

[28] See "Comments and Criticisms," this JOURNAL, *loc. cit.*, p. 130.

[29] I find it difficult to suppose that human beings might continually employ means appropriate to a certain aim, and thus "have the aim," and yet lack belief in the finite probability of success.

not yet been dealt with. To insist that conditions (*a*), (*b''*), (*c'*) constitute a justification of an instrumental procedure would amount to maintaining that if these conditions were fulfilled for any person, he ought to act as if condition (*b*) were fulfilled. Professor Reichenbach's contention that the existence of belief in the possibility of success is irrelevant to his justification seems to result from interpreting conditions (*a*), (*b''*), (*c'*), as giving us another type of attenuated justification. If this is Professor Reichenbach's view then the following points should be made clear. First, our desires or aims are pretty closely connected with our beliefs about the practical possibility of success, and behavior conforming to the "ought" in question would at least lead to the psychological phenomena indicative of a state of belief. In other words, to conform to the "ought" in question would be to act in such a way as to produce the groundless belief in the possibility of success. Secondly, the type of justification under consideration differs from the type which follows from conditions (*a*), (*b'*), (*c'*), in this way only. Instead of a justification of a choice of means as appropriate to a groundless belief and aim, we now have a justification of a choice of means as appropriate to a groundless aim. Such a justification presents some resemblance to James's doctrine of the "Will to Believe." The voluntarism of James tends at times to make a virtue out of the fact of behavior which in the strict sense is not rationally justified. Still, James's doctrine is an antidote to an unrealistic view which would suppose that *all* behavior might be rationally grounded and would forget that belief in success is itself a causal factor in achieving ends. There is and has to be behavior which can not be justified in the strict sense, both in practical life and in the pursuit of science, and more obviously in the former than in the latter. In practical life, short of the pervasive behavior-pattern which is inductive inference, we are often enough in situations in which we have to act (refusal to "act" being a species of action) and in which our knowledge of the practical possibility of success approaches ignorance. We may flip a coin to determine the decision as to whether or not we will essay a certain end or we may let our emotions guide us. The answer to James is *not* to counsel an impossible suspension of judgment and action. The answer is twofold:—(*a*) frank recognition of the fact of behavior which can not be rationally grounded in the sense indicated; (*b*) refusal to lend the decision to undertake such a course of behavior when determined by desire or want of an end, rather than by the flipping of a coin, an air of pseudo-rationality, by suggesting that in some profound sense of "ought" we "ought" to hearken to the heart when the head can not guide us. The "counsel" appropriate to this sort of situation is not,

I think, an imperative that claims to follow from a justification before the intellect, but rather some such remark as, "Well, if you want to do it so much, go ahead and try. And as long as you *are* going to try, try in this way, because it's the best way to get what you want—*if* that is possible (practically)."

The third type of remark, to which I now turn, has suggested to critics that something like Pascal's wager lurks behind Professor Reichenbach's imperative. At times what Professor Reichenbach seems to mean by stating that inductive procedure is our best posit or wager is merely the tautological demonstration that if there are limits to the frequencies of series of events, then the inductive procedure is a sufficient condition of success. However, at other times, as in the remarks quoted, it is suggested that "best posit" here is used in a broader sense as including the factor of the value of success for the scientific gambler and that when this factor is introduced and we realize that the probability of success is unknown, or better, indeterminate, an imperative to place the bet is warranted. The value of the posit or wager is, of course, the arithmetical product of (*a*) the probability of success, and (*b*) the value which success would have for the wagerer. The wager with the greatest weight for success is not always our best wager if we take into account the fact of value. Thus, if the respective values attached to events of different probabilities differ in a ratio which is greater than the inverse ratio of the probabilities, the "best wager" is the wager on the less probable event. Though Professor Reichenbach states [30] that he uses "best posit" in the narrower sense which excludes considerations of value, the inductive procedure is called our "best wager" as well as our "best posit" and in view of the remarks quoted above, it seems that considerations of value underlie the assertion that we ought to employ inductive procedure. If this is so, a new element enters into the "justification" of induction, and an element which does at least in some ways resemble Pascal's wager. The probability of success is unknown, but we do know (*a*) that if success is practically attainable, laying the wager is a necessary condition of attaining it, and we do know (*b*) the value that success would have, and in view of these known factors we are commanded to bet. In such a situation the reasonable thing to do (in the attenuated sense of reasonable, to be sure) is to employ the best means. But it must be noted that the justification of a course of action in this type of situation is dependent upon the psychological or biological fact of human preference. No imperative can be asserted for all men in such a situation unless a material proposition about a certain unanimity of human likes be established as highly probable.

[30] *Experience and Prediction*, p. 316, note.

Moreover, Professor Reichenbach seems to forget that the relevant value factors are not exhausted in "the certainty of getting nothing" and "the possibility of getting something." To choose the way that leads to success if success is possible, is also to choose the way that *is*, if success is impossible, a life-long search for a nonexistent object. To choose the way that does not lead to success, if success is possible, is also to choose the way that is *not*, if success is impossible, a life-long search for a nonexistent object. That we do take the former way, though at times fitfully, blindly, and inefficiently, is due, not to an intellectual calculation of values, but to gentle force of the habit of induction.

To summarize my conclusions in regard to the "justification" of induction accomplished by Professor Reichenbach,

(1) The justification of inductive procedure can be analogous only to an *attenuated* type of justification of instrumental action.

(2) Justifications of instrumental actions are always conditional. An imperative can not be asserted, and hence in this sense the justification actualized, until certain conditions are established.

(3) Professor Reichenbach's tautological demonstration establishes only one of three conditions requisite to asserting a weak sort of imperative.

(4) According to his own admission, to establish one of the other conditions would be to establish a synthetic proposition about human aims.[31] The content of this proposition seems to me to resemble closely one aspect of Hume's account of human habits and to imply [32] the remainder.

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ON THE JUSTIFICATION OF INDUCTION

MISS CREED'S paper gives me the welcome opportunity to explain more fully what I understand by a justification of induction. We both agree that the question of justification can be raised only if a decision on a certain aim has been made; i.e., justification concerns the question of the appropriateness of a certain means in respect to a chosen aim, not the question of the choice of the aim itself. The question of appropriateness, however, includes the question of the attainability of the aim; thus we have to distinguish different kinds of justification according to differences in

[31] The assertion of this proposition would, of course, presuppose the inductive assumption.

[32] Whether or not the implication is taken as a strict one depends upon whether or not one takes a behavioristic view of belief.

what we know about the attainability of the aim. I say "differences in *what we know*," for a man's actions can be guided only by his knowledge of the world, and not by unknown features of the world; if we have to decide whether his actions are reasonable we have to ask whether they are reasonable in relation to what he knows.

Now as to our knowledge of the attainability of the aim I would distinguish the following cases:

1. We know something about the objective possibility of reaching the aim A by applying the means M. We can introduce here the subdivisions:

α we know that by applying the means M we shall certainly reach the aim A;

β we know the probability p that A will occur if M is applied;

γ we know at least that $p > 0$;

δ we know that although p may be $= 0$ the aim is possible.

The difference between γ and δ is here not very great; we have to distinguish these cases only because the probability $p = 0$ is not equivalent to impossibility (i.e., the limit of the frequency may be 0 although the event sometimes happens). For the same reason case α is slightly different from the case $p = 1$. But these differences are not very important for our problem.

2. We do not know whether or not application of the means M will lead to the aim A.

3. We know that by applying the means M we shall never reach the aim A.

Now we certainly agree that in case 3 a justification of the procedure M can not be given. But it seems to me that in all the other cases it can be given. Although we know only in case 1α that the means will lead to success we should call the procedure justified also in the cases 1β, γ, δ, and 2, if the means M can be shown to be the best means we know in respect to A. Miss Creed would agree with me as far as $1\alpha, \beta, \gamma$ are concerned; her condition b is equivalent to my case 1γ. The difference then would be that a justification is not considered possible in the cases 1δ and 2.

Instead Miss Creed considers another case which is called b' and defined by the clause "that it be assumed or believed that success is practically possible, though there be no evidence to support the assumption or belief." I confess that this seems to me a rather strange case. Belief can be the *motive* of an action, but belief as such can never *justify* an action; only a *justified* belief can do that. But if according to the definition of case b', the belief is unjustified—how can it lead to a justification of induction?

This is a point in which, it seems to me, Miss Creed's paper re-

veals the tracks of David Hume, and unfortunately of that part of his theory of induction which has brought so much confusion into the empiricist camp. It seems to me that after his brilliant criticism of induction, the merits of which can not be overestimated, Hume ran the problem into a side track by his defense of inductive belief as a habit. Even today the consideration of this habit, and belief, is in the foreground of the philosophic discussion of the inductive problem, and eclipses the logical problem behind this psychological fact. Miss Creed speaks in the title of her paper of the justification of the *habit* of induction. I do not think she would ever have written a paper on the habit of the syllogism. Although syllogistic inference is a habit also, as well as inductive inference, nobody would mention this fact within a logical analysis. Unfortunately, ever since David Hume's turning of the problem of the inference into the problem of a habit logicians have shared his escape from logic into psychology. But unwarranted belief, even if it is a habit, can never confer any title of justification to the scientific procedure of induction.

So what we have in case b' is, logically speaking, nothing but the clause added, that there is no evidence against the possibility of success. But this is exactly my case 2, the very case which corresponds to the logical situation of the problem of induction. The fact that even in this case a justification can be given, though overlooked in the traditional exposition of the problem, seems to me to open the way to a solution of the inductive problem.

The situation is obscured by some ambiguity in the word "possible." We have to consider first *objective possibility;* by this term we mean cases in which an event sometimes happens. For instance, we say it is possible that it will rain tomorrow, and mean by this that there are summer days on which it rains. Besides this, there is what we might call *epistemic possibility*, i.e., the case that we know the objective possibility. Whereas the terms "objective possibility" and "objective impossibility" form a complete disjunction, the epistemic terms lead to a trichotomy: either we know the possibility of an event, or we know the impossibility, or we do not know anything about the possibility or impossibility. Thus "not impossible" in the epistemic sense is not equivalent to "possible"; it would include the case of indeterminacy. Sometimes, however, we understand by epistemic "possibility" the same as the epistemic "not impossible," that is, we include the case of indeterminacy into the term "possible." I think that if somebody insists that a justification should include a demonstration of the possibility of success he means "possibility" in this sense. If this is meant I would, of course, agree with such a postulate. It is clear that in this sense success of the inductive procedure is possible.

I turn now to a consideration of condition *c*. Miss Creed is certainly right in saying that if we speak of the justification of a means, or procedure, this justification is conditional in so far as it refers to a certain aim. If somebody does not want the aim in question an application of the means by him would not be justified. But it would mean misunderstanding my theory of induction if it is said that for a justification I should have to prove condition *c*, or *c'*, both of which certainly are synthetic propositions of a psychological content. It is not my task to prove these propositions; they stand in the implicans of my theorem, and all I want to maintain, and need to maintain for a justification, is an implication of the form: if a man fulfills condition *c*, or *c'*, i.e., if he wants to predict, he should make inductions.

But apart from this easily corrigible misunderstanding I readily consent to Miss Creed's very pertinent distinction between *c* and *c'*, between the case of wanting the aim in general, and wanting it in face of the more or less problematic chance of success. It is quite right that my implication presupposes *c'*, and that *c* is not sufficient. I am glad Miss Creed pointed this out so clearly; my writings are perhaps not clear enough on this point although I never wished to maintain anything else.

It seems, however, Miss Creed considers condition *c'* unsatisfactory. She would prefer to have a method which permits us to deduce *c'* from *c*. This refers to the methods using the concept of "mathematical expectation" which determine whether or not a bet is acceptable. If *a* is the amount of pleasure attached to the aim *A*, *m* the amount of displeasure combined with the application of the means *M*, *p* the probability that *A* will occur if *M* is applied, then the mathematical theory furnishes the result that the bet is acceptable if the "valuational balance"

$$p \cdot a - m$$

is greater than 0. This condition is easily deducible if *a* means the amount of money to be gained in a game, *m* the amount of money to be paid in advance, i.e., the individual stake. We sometimes extend this condition to cases in which *a* and *m* mean emotional values, assuming that there is a possibility of measuring them. Only such a calculation of the valuational balance, so far as I see, would be considered by Miss Creed as a perfect justification of induction; if she denies to my theory of induction the predicate of justification it seems to be because I do not give an analysis of this kind. That is, I am invited to prove that the pleasure of successful prediction multiplied by the chance of success exceeds the displeasure of the troublesome procedure of induction.

Now I will not deny that in some cases of volitional decisions we engage in considerations which at least qualitatively may be represented by the mathematical calculation described. However, such an analysis of the volitional decision to act is not always possible; and even if it is possible it mostly is not relevant. It is not possible if p is not known; thus already in case 1γ it is not possible unless we are ready to consider a as an infinite value which seems to me rather absurd. And if it is possible it is not relevant because it does not free the volitional decision from a subjective element. That is, instead of having in the implicans the subjective decision to act, we have there now the subjective constants a and m; and if a man rejects our suggestion to act he may always defend himself by saying that his emotional constants do not furnish a positive valuational balance.

Moreover, it is very dubitable whether we are entitled to assume the existence of such emotional constants as a and m. Emotional values vary widely with the situation. The means M may appear very unpleasant to us if considered in isolation but if we know it is the means to reach the coveted aim A our dislike for M may turn into a liking for it. Any intellectual calculation of the valuational balance does violence to the actual psychological processes by which the volitional decision is made. What actually happens in most cases is that when confronted by the situation, we perform a decision *en bloc*, i.e., we perform an act not analyzable into the components p, a, m. Thus Miss Creed's condition c' is a much better representation of the actual situation than would be a deduction of c' from c.

I may add here the remark that even if such a calculation of the valuational balance could be given, a positive value for this balance would not involve an obligation to act. Our man might object that he accepts a bet only if the valuational balance is greater than a certain valuational threshold t. This is, for instance, the practice of insurance companies which calculate the premiums so that statistical laws guarantee to them a profit. Or our man might object that he accepts a bet only if the probability p is greater than a certain safety quantity s; if p is smaller than s he considers the event as practically impossible. The case then would be better represented by case 3 in which the action is not justifiable. I think this is a principle which we all more or less apply because "impossible" never means for us more than "practically impossible." If, for instance—to make use of Miss Creed's interesting example—we refuse to recognize an obligation to comply with the rites of a certain church in spite of there being a slight chance of truth in the promise that such a behavior would lead us to eternal

happiness, this negative decision I think is best explained by the introduction of the principle that a probability of so small a degree is considered as a practical impossibility. (It follows, by the way, that cases 1γ, δ, and 2 will permit a better justification than case 1β, if in the last case p is known to be small; knowing nothing about p is a less unfavorable situation than knowing that p is small.) With these considerations two further subjective constants t and s are introduced, which make the relation between wanting the aim in general, and deciding under given conditions for an action in its favor, more flexible and questionable.

On the other hand there are cases in which the calculation of the valuational balance is unnecessary because its positive value can immediately be asserted. This is the case if m is not a displeasure but a pleasure; the sign of the m-term in the valuational balance then would be positive. There are many examples of this kind. Imagine the father saying to his son: If you want to have a car of your own take the old car in the garage and repair it. The boy may be glad to have, not only a car, but also the opportunity to repair a car. This case, I suppose, corresponds best to our actual behavior in face of the problem of induction. I think most of us would not envy the lazy man who waits until the fried pigeons fly into his mouth; we like to work, to try success, to gamble—at least as long as we like to live. I do not say this with the intention of giving an exhaustive psychological analysis of the decision to attempt to predict. I should have no objection if one should speak here of the great contribution which habit adds to our decision to act, for this is a point where we have left the logical field and are concerned with the psychological motives of our actions. Habit, though never a permissible argument for the choice of the *means* of prediction, may well be accepted as the motive which determines the *aim*. Miss Creed speaks so charmingly of the "gentle force of the habit of induction" which leads us, instead of "an intellectual calculation of values"; let us consider this idea together with the joy of gambling as the psychological motives of our decision to act. But let us carefully distinguish between the psychological question of the motives of our decision, and the logical question of the best means we can choose in order to reach the aim of our decision.

This logical question remains unchanged whatever may be the result of the psychological analysis as to the motives of our pursuit of the aim. Even if the application of the means M is combined, not with displeasure, but with pleasure, the question whether it is an appropriate means for the aim A has to be decided independently of this psychological fact. M involves pleasure for us only because it is the way towards A.

I shall summarize. The problem of the justification of induction includes both the question of the decision to attempt predictions and the question of the choice of the best means of making them. The decision on the attempt is justified if the aim is not proved to be unattainable. To demand more, to show that the aim will be reached with a probability p and that the valuational balance is positive would be equivalent to an analysis of the volitional decision which seems inappropriate from a psychological viewpoint and which would not eliminate the subjective element reappearing in the emotional constants a, m, t, s. I therefore can not consider such an analysis as necessary for a justification; a decision *en bloc* is permissible in the implicans of our proof. The word "justification" is chosen with the very intention of indicating the subjective element; otherwise we should speak of a demonstration of an *obligation* to pursue the aim. Only when we turn to the second point are we confronted by a problem free from subjective elements, as the question of the choice of the best means is purely logical. Justification of the choice of the means is therefore equivalent to the logical demonstration that the means is the best we have. If this proof is given the problem of justification is solved, since it is obvious that the first condition, the lack of a disproof of the possibility of success, is fulfilled.

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BOOK REVIEWS

The Kantian Philosophy of Space. CHRISTOPHER BROWNE GARNETT, JR. New York: Columbia University Press. 1939. xii + 287 pp. $3.50.

The voluminous literature on Kant's philosophy contains many discussions of his doctrine of space. Works like Vaihinger's *Commentar zu Kants Kritik der reinen Vernunft,* and the writings of Ernst Cassirer, notably *Das Erkenntnisproblem,* are virtual sourcebooks on this problem. But with the exception of these works and a number of obscure monographs, mostly German, Kant's philosophy of space has been treated usually as one problem among many others in the *Critique of Pure Reason.* Professor Garnett's study is unique in that he undertakes an examination of the sources of Kant's doctrine of space in Newton, Clarke, Leibniz, and Euler (the latter in passing only), then traces the development of Kant's doctrine from the early scientific works through the "Analytic" of the first *Critique,* and concludes with some brief

reflections on the relation of Kant's conception of space to those of Alexander, Whitehead, Broad, and, incidentally, of Einstein. To trace a problem of such import through two and a quarter centuries of scientific and philosophical controversy, and to undertake, as Professor Garnett does, a sharply critical evaluation of the complicated ideas and permutations of doctrine involved, calls for extraordinary erudition and no small measure of courage. That this task could not be accomplished exhaustively and with finality in a slender volume of under three hundred pages, seems a foregone conclusion.

For purposes of review the book may be divided into three parts. The first of these surveys the chief sources of Kant's doctrine of space. "Sir Isaac Newton," the author maintains, "had no considered philosophy of space. . . . In spite of this fact, the theories of natural philosophy and mathematics of Newton were of greater influence upon Kant's philosophy of space than the more explicit philosophies of space of his other predecessors" (p. 11). Newton's doctrine of space, we are told, did not rest on the inductive method, it was not a necessary consequence of the laws of motion, it owed but little to Newton's theology. The reader is left to conclude, apparently, that it rested on nothing at all. Yet it proved highly instructive to Kant, particularly through the controversy of 1715 in which Samuel Clarke defended Newton's view, not always consistently, against that of Leibniz. In the Newton-Clarke view, space, being a "property" of God, is absolutely infinite and can not be known through ideas or conceptions. Against this interpretation Leibniz held that space is neither substance nor property, but a relation, "metaphysically unreal" and altogether abstract. Kant incorporated elements of both views into his doctrine. Thus by 1769 he had concluded that space is an infinite whole, not composed of parts, known directly through intuition alone, and metaphysically ideal.

The discussion of the sources concludes with a chapter, "Newton vs. Leibniz on Perception," in which the author does not remain strictly within the bounds of what is relevant and illuminating for his main thesis. The problem of this chapter, the nature of our knowledge of space and the relation of perceptions to space, is obscured by a tendency to evaluate the points under discussion by reference to undeclared assumptions of contemporary epistemology. Both Newton and Leibniz are reproved for their lack of realism in epistemology—as though realism, supposing even it were clear what that means, were the only consistent and acceptable position for a respectable philosopher. In this connection the author launches into a general critique of subjectivism, the perti-

nence of which to the main argument is far from clear. One is expected also to accept rather too much on the author's unsubstantiated claims. Samuel Clarke, for example, is accused of having unconsciously falsified the meaning of Newton's doctrine of the relation of space to God; but the passages from Newton and Clarke which would convict the latter are not brought to bear on the question. Leibniz, too, is subjected to some dubious handling; his theory of perception is considered with but scant reference to its metaphysical context and judged by criteria quite foreign to his system. The argument of much of this chapter is ineffective and unconvincing, largely because the author tends to make of the examination of each problem an end in itself. The reader is left in doubt as to just what is at stake and precisely what conclusions flow from the belabored analyses.

It was not the author's intention to make the survey of Kant's sources complete or exhaustive. Relatively little attention is given to the influence of Euler, none at all to such lesser figures as Crusius, Keill, and Freind. Certain limitations of Professor Garnett's interpretation may be traced in part to the failure to examine the influence of those continental scientists who weaned Kant from Wolffian rationalism and directed his attention to the empirical foundations of science and the problem of method.

In the second and most important part of the book, Professor Garnett expounds and attacks in order, Kant's very early views of space, his doctrine of space "about 1769," the space of the "Aesthetic," and the space of the "Analytic." The author essays at one and the same time (*a*) to expound Kant's doctrine in each of these stages, (*b*) to determine the probable roots and relations of each stage, (*c*) to trace the permutations which the ideas undergo and the probable motives therefor, and (*d*) to evaluate Kant's doctrine in each stage and as a whole.

The general thesis which emerges foggily from pages of labyrinthine reasoning is this: In the early pre-critical writings on space Kant had defended doctrines which were incompatible with his mature critical position; yet he dragged the early ideas into the *Dissertation* of 1770 and into the "Aesthetic" in both editions of the first *Critique*. In other words, Kant's doctrine of space does not become consistently critical previous to the "Analytic." Taken as a whole, therefore, Kant's philosophy of space is a congeries of elements of which some are untenable, some mutually inconsistent.

This thesis, which has much to recommend it, has been argued with acumen and balanced judgment in *A Commentary to Kant's "Critique of Pure Reason,"* by Norman Kemp Smith. It is to

him that Professor Garnett dedicates his book, and justly so; for it aims to establish in detail several criticisms of Kant's doctrine of space which Professor Smith had directed against the Neo-Kantians. In the execution of his task Professor Garnett seeks to establish his interpretation by an appeal to evidence which is at best always questionable and at worst downright contrary to his thesis. Much of the debate centers about the interpretation of Kant's *Inaugural Dissertation* of 1770. Most of the great German commentators, the Neo-Kantians in particular, regard this work as the decisive opening gun of Kant's "Copernican Revolution." While Professor Garnett acknowledges this "Wendung von 1769" in a general way, he minimizes its influence wherever it pertains to Kant's doctrine of space. With Hamlet thus out of the play, the author has little difficulty in making Kant's doctrine look like a sorry patchwork indeed.

To be sure, Kant laid himself open to such handling; for even in his critical philosophy he did not always distinguish clearly between the psychological and subjective conditions of apprehension, and the logical presuppositions of experience. The author makes the most of this confusion. Kant, he points out, can not maintain with consistency on the one hand that space is an intuited objective whole (*totum analyticum*), as he does in the "Aesthetic," and hold on the other hand that space is generated or constructed (*totum syntheticum*), as in the "Analytic." Since the latter position is unassailably critical, the author sets out to expose every possible flaw in Kant's doctrine of intuitions. To begin with, Kant's doctrine of intuitions, the author urges (p. 134), did not result from the effort to find an *a priori* necessary foundation for mathematics as Vaihinger and numerous other commentators have contended, but originated instead in reflection upon the significant differences between space and general concepts.

In connection with this point, the author puts his finger on many dubious and controversial elements in Kant's doctrine. The quality of the author's reasoning may be judged by the following sample. In order to discredit Kant's doctrine of intuitions altogether, the author proposes to show that in 1770 Kant subscribed to the possibility and validity not only of sensuous intuitions but of intellectual intuitions as well. In Kant's *Dissertation,* Professor Garnett contends,

> . . . Two clear and self-consistent, but contradictory theories of knowledge flow from the assumptions (1) that man has intellectual intuitions, and (2) that he lacks them. Confusion results from Kant's attempt to combine the second contention with conclusions flowing from its contradictory, the first. [P. 153.]

In proof of "(1)" the author quotes (p. 153) from Kant's *Dissertation* the following: "We rightly suppose that what can not be known through any intuition whatever is not thinkable at all, and so is impossible."[1] From this statement, which says nothing directly about intellectual intuitions, the author infers that since Kant speaks (in other connections) of non-sensuous ("real") objects, there must be human non-sensuous intuitions through which these are apprehended. Unfortunately, in the Kantian text quoted by the author, the very next sentence, which to the reader's astonishment Professor Garnett quotes later (p. 158, note 60) in support of "(2)," guards explicitly against this inference. Kant's text continues:

> But since by no effort of the mind can we attain even in imagination another kind of intuition than that which takes place according to the form of space and time, it comes about that we regard as impossible all intuition whatever which is not bound by these laws (neglecting the pure intellectual intuition which is exempt from the laws of the senses, such as the divine intuition which Plato calls idea), and so subject all possible things to the sensitive axioms of space and time.[2]

In other words, if we are to accept Professor Garnett's point on the evidence presented by him, we must convict Kant of flagrant and complete contradiction in successive sentences—sentences which define the essentials of his critical philosophy.

A scrupulous reading of Sections IV and V of the *Dissertation* reveals that Kant's definition of the intelligible world did not entail the acceptance of human intellectual intuitions.[3] It was a doctrine of method simply—the critical or transcendental method, here contained in embryo but developed years later in the first *Critique*. One of many passages in the *Dissertation* may be quoted in substantiation of this claim. "In pure philosophy," Kant asserts, "such as metaphysics, in which the use of the intellect in dealing with principles is real, that is to say, in which primary concepts of things and relations, and axioms themselves, are primarily given by the pure intellect itself, and which, *since they are not intuitions,*[4] are not immune from error, *method precedes all science.*"[5]

[1] § 25, Handyside trans., p. 75.

[2] § 25, Handyside trans., p. 75. The exception which Kant makes in the parenthetic statement clearly refers only to divine, i.e., non-human intuitions, if there be such. See also § 10, p. 50 *ff;* and § 12, p. 51.

[3] Excepting that in so far as Kant was already looking ahead to his final practical philosophy, he was prepared to admit the knowledge of the *"Perfectio Noumenon"* in the pure intellect. *Dissertation,* § 9, p. 49. This doctrine in its mature form became "the postulates of practical reason."

[4] These italics my own.

[5] § 23, Handyside trans., p. 72.

The third part of Professor Garnett's study, a brief last chapter, surveys the space theories of S. Alexander, Whitehead, and Broad with a general indication of the relation of each to Kant. Alexander's view, the author concludes, is more like that of the "Aesthetic"; Broad's theory is compatible only with the "Analytic." Whitehead's doctrine is nearer Leibniz than Kant. These pages contain little more than a sketch of a very large subject which could not be handled satisfactorily short of another book.

In the last pages the author resumes the attack upon the Neo-Kantians, in particular on certain conceptions attributed to Kant by Ernst Cassirer in his *Substance and Function.* In the latter's interpretation, space meant for Kant "a serial form of co-existence and of succession." As such it is equally compatible with Newtonian or modern relativistic conceptions. Of this view Professor Garnett writes, "It seems to me idle speculation to claim that Kant's doctrine of pure intuition meant from 1769 to 1787 what it must now mean if it is to receive confirmation from present-day physical teachings" (p. 259). In his impatience to be done with the Neo-Kantians the author holds them too cheaply. Strictly speaking, no empirical account of space could "confirm" Kant's doctrine. With all the looseness of Kant's terminology, his complexities and inconsistencies in matters of detail, the original and characteristic position which Kant developed from 1769 to the end was the critical or transcendental doctrine and method. The author does not in this work exhaustively appraise Kant's doctrine of space in the light of this general method, but proceeds instead to splinter Kant's ideas by means of gimlet-eyed analyses of the psychological and subjective moments in Kant's exposition. While this has its uses, it seems to the reviewer to miss the wood for the trees. Cassirer's position, on the other hand, seems not only to rest upon the main line of evidence as well as on Kant's repeated declarations of what was new and revolutionary in his own thought, but has the additional merit of leading to a positive and fertile interpretation of Kant's philosophy.

The shortcomings of this book are certainly not traceable to a poverty of ideas or a lack of critical acumen. On the contrary, its outstanding merit lies in the number and range of the problems it opens, the wealth of aspects from which each problem is viewed, the intricate relationships which it uncovers, the inconsistencies and ambiguities it detects. For these reasons a thorough student of Kant's philosophy can ill afford to neglect this work. But the book suffers the defects of its virtues. It does not succeed in giving the reader a clear conception of the subject or of the direction in which the argument as a whole is moving. There is so much

less of understanding and illumination in the book than the reader will hope for. A turbid style and needlessly involved manner of exposition greatly augment these difficulties.

In the opinion of the reviewer, *the* book on Kant's philosophy of space still remains to be written. But whosoever undertakes to write it will profit greatly from a study of Professor Garnett's book.

OTTO F. KRAUSHAAR.

BOOK NOTES

Das Einzelne bei Platon und Aristoteles. ANDREAS PREISWERK. (Philologus: Supplementband XXXII, Heft I.) Leipzig: Dieterich'sche Verlagsbuchhandlung. 1939. ix + 196 pp. 11 M.

By means of a detailed comparison of the later Dialogues with *Metaphysics* Z, it is shown how Plato's "indivisible" (ἄτομον εἶδος) differs from Aristotle's "individual" (τόδε τι), regarded as form immanent in matter (ἔνυλον εἶδος). The former functions as an ideal limit between the field of exact knowledge, consisting of a logical hierarchy of forms, and the indeterminate range of the sensibly given, the *Apeiron*. As such, the individual expresses in itself the whole order of which it is the ultimate terminus. Aristotle, however, takes the individual to be an independent datum and so it requires no formal derivation from a higher order of knowledge or being.

In its main argument, the book suggests the views of Professor Jaeger and the late Julius Stenzel.

D. S. M.

Geschichte des Hegelianismus in Russland. Vol. I. BORIS JAKOWENKO. Prague: Josef Bartl. 1938. xii + 349 pp.

This detailed account of the reaction of Russian writers to Hegel's philosophy in the decade 1838 to 1848 is interesting, but in one sense incomplete. While the rise and decline of Hegel's influence on (M.A.) Bukunin, Belinsky, and Herzen is closely documented, and their development from purely theoretical to more practical interests is traced in books, periodicals, and correspondence with much scholarship, almost nothing is said of the nature of the practical interests which motivated this intellectual development. On the other hand, documentary material is given on special questions, such as Bakunin's relation to the Left-Hegelians. A short section describes the influence of Hegel on the Pan-Slavists. Leading men such as Turgenev and Dostoevsky, and many minor

figures are brought into the picture. Two later volumes will bring the story of Hegel's influence on Russian writers up to date.

V. J. McG.

Trattato d'estetica. MARCO TREVES. (Problemi Filosofici, 3.) Firenze: "La Nuova Italia." 1938. 319 pp.

Signor Treves has written the kind of book to which French and Italian estheticians—but particularly the latter—seem much given. The temper is intimated in the preface, which distinguishes the method of science: "it must begin with axioms so evident that they enjoy universal consent"; "it must proceed by valid inferences, not by sophisms"; "it must attribute to each term a constant meaning throughout the whole work."

The author wishes to make esthetics a "science" in this sense, and his book is an exercise in assumptions offered as self-evident axioms, terms defined with less concern for a subject-matter than for the clear pleasure of definition. In short it is the construction of a vocabulary rather than the analysis of a problem.

The vocabulary is not without interest: Beauty, Fantasy, Sentiment, Poetry and Non-Poetry (in the text, so far as one can make out, a peculiarly dialectical distinction). The author is apparently not without cultivation in the arts, and he makes now and then suggestive by-the-way remarks. But not at one point in the text, not even in the reasonable attempt to distinguish the arts from each other, does one sense anything but a concern with the internal relations to each other of terms used primarily for their manipulability dialectically, rather than for their reference to the arts and the work of artists.

I. E.

OTHER NEW BOOKS AND JOURNALS

Müller, Gustav E.: Hegel. Über Offenbarung, Kirche und Philosophie. München: Ernest Reinhardt. 1939. 60 pp.

Levy, Jacob: Space Moves thru Time. New York: Privately Printed. 1939. 93 pp. (What appears to be the thesis of this pamphlet is stated on page 2: "Space constantly moves from the instant of time when it contains one arrangement of stars to the future instant of time when it contains a successive and different arrangement of stars!" The rest consists of a dialogue constructed on the principle of free association.)

Portraits of Famous Philosophers Who Were Also Mathematicians. With Biographical Accounts by Cassius Jackson Keyser. New York: Scripta Mathematica. 1939. (The dozen excellent portraits contained in this collection are those of Pythagoras, Plato, Aristotle, Epicurus, Roger Bacon, Descartes, Pascal, Spinoza, Leibniz, Berkeley, Kant, and Peirce. Each biographical note contains a brief outline of the philosopher's life and of the scope of his interest in the mathematics of the day.)

MIND. Vol. XLIX. No. 193. The Philosophy of Samuel Alexander (I): *G. F. Stout.* Perceptual Assurance (I): *D. R. Cousin.* Development within Berkeley's *Commonplace Book: A. A. Luce.* Discussions—Did Berkeley Misunderstand Locke?: *W. H. F. Barnes.* On Von Mises' Theory of Probability: *R. L. Goodstein.*

TIJDSCHRIFT VOOR PHILOSOPHIE. 1e Jaarg., Nr. 3. Aristoteles en het vrijheidsprobleem: *A. Mansion.* Phénoménologie et dialectique de la tolérance: *Gabriel Marcel.* Newman's beschrijving en verantwoording van het werkelijk denken: *H. Walgrave.*

We have received the following reprints:

"The Fundamental Opposition of Plato and Aristotle" by Erich Frank. Reprinted from *American Journal of Philology,* Vol. LXI (1940), pp. 34–53 and continuation in a later issue.

"Aristotelianism in Western Christianity" by Richard McKeon. Reprinted from *Environmental Factors in Christian History,* University of Chicago Press, 1939, pp. 206–231.

"Tocqueville's Philosophy of Freedom" by Albert Salomon. Reprinted from *The Review of Politics,* Vol. I, No. 4, pp. 400–431.

"The Crisis in the Marxian Theory of Law" by N. S. Timasheff. *Contemporary Law Pamphlets,* Series 2, Number 1 (1939). New York University School of Law.

"Is Democracy Right?" by Edgar S. Brightman. Reprinted from *World Affairs Interpreter,* Vol. IX (1938), pp. 265–274.

"Chaos and Cosmos" by Edgar S. Brightman. Reprinted from *Religion in Life,* Winter Number, 1939, pp. 16–30.

"L'uomo, la storia e la morale" by Teodorico Moretti-Costanzi. Reprinted from *Logos,* Anno XXII (1939), pp. 5–20.

"Un cenno sulla gnoseologia pura" by Teodorico Moretti-Costanzi. Reprinted from *Criterion,* Anno VII (1939), fasc. 1–2.

"L'infinito" by Giulio Colesanti. Reprinted from *Ricerche Filosofiche,* Fasc. III–IV (1939).

NOTES AND NEWS

We regret to learn of the death of Edward G. Spaulding, Professor of Philosophy at Princeton University, on January 31, at the age of 66 years.

We regret to learn of the death of David S. Swenson, Professor of Philosophy at the University of Minnesota, on February 11, at Lake Wales, Florida, at the age of 62.

A trust fund for "the promotion and encouragement of philosophical and scientific research throughout the world" was established by the will of Dr. Charles Augustus Strong. Bertrand Russell, G. E. Moore, and Julian Huxley were named trustees of the fund to be known as the Philosophical Fellowship. Its purpose is to be carried out through the establishment of separate fellowships for scientific and philosophical study in the United States, England, and France, under the direction of the trustees, from the interest on stocks and bonds, with an approximate value of $133,500 listed by the testator.

We have received notice of the appointment of Professor Etienne Gilson as Visiting Professor of Philosophy on the Mahlon Powell Foundation at Indiana University. He will deliver a series of four lectures at Bloomington, Indiana, during the week of March 3, 1940, on the general subject "God and Philosophy." The topics of the individual lectures are (I) "God and Greek Philosophy," (II) "God and Christian Philosophy," (III) "God and Modern Philosophy," (IV) The Contemporary Problem of God."

Charles W. Hendel, Dean of the Faculty of Arts and Sciences at McGill University, has been appointed Professor of Philosophy and Chairman of the Department of Philosophy at Yale University.

The trustees of Union College have voted to establish the Ichabod Spencer Professorship of Philosophy, and have announced the appointment of Professor Harold A. Larrabee, who has been at Union since 1925, as its first incumbent.